

it so happens that I was present, and can testify to the accuracy of Mr. Mello's statement. With regard to the tooth of Machairodus, which I discovered and afterwards showed to Mr. Heath, it is asserted that it was without adherent matrix, and without the moisture which it would possess had it been imbedded in the cave for ages. These assertions are disproved by the facts that the tooth unfortunately split in pieces in process of drying, and that the matrix of red earth, only partially removed when it was repaired and gelatinised in the Owens College Museum, is still to be seen in the pulp cavity.

In the exploration of the caves, in 1876, the discoverer, Mr. Mello, was director, while I undertook to name and classify the remains, and we drew up a report published in the *Quart. Geol. Soc. Journ.*, 1877, p. 475. Mr. Heath and myself acted as superintendents of the work, under the direction of Mr. Mello.

It was Mr. Heath's duty as superintendent to hand over to the director the notes on which the above assertions are based, as well as any other notes relating to the work entrusted to him. He did not do so. If he had any fault to find, it was his duty to lay it before the committee, and in the interest of truth to make his statement when the report was read at the meeting of the Geological Society, at which he was present. He did neither of these things. Nor when he had many opportunities of saying what he liked at the meeting of the British Association at Sheffield, after my paper before the Geological Section, and our addresses at Cresswell, did he say one word, although he was present at both. The pamphlet in question was to us the first intimation that he differed with us as to the facts.

In conclusion it only remains for me to add that Mr. Heath was *not* a member of the Exploration Committee, that he was merely a subordinate to Mr. Mello, and that, on his own showing, he kept back for nearly three years notes considered by him to be valuable, which he was in honour bound to communicate at once to the director for the information of the committee—notes which were as much the property of the committee as the fossil remains discovered in the caves at their expense. I am instructed that the only notes which he gave to the director were certain measurements of the inside of the Robin Hood Cave, which it was found necessary to have done over again.

W. BOYD DAWKINS,

Secretary of the Cresswell Caves Exploration Committee, 1876

"The Society for the Encouragement of Literature and Science"

THE attention of the Council of the Geological Society has been called to the prospectus of a "Society for the Encouragement of Literature and Science," in which the letters "F.G.S." are appended to the name of one of the vice-presidents and to that of the "Secretary-in-Chief." I have been directed by the Council to make it generally known that neither of these gentlemen is a Fellow of the Geological Society, as would naturally be inferred from their use of these letters, and I shall feel greatly obliged by your insertion of this note in your columns.

Geological Society,

W. S. DALLS,

Burlington House, November 27

Assist. Sec. Geol. Soc.

THE attention of the Council of the Linnean Society of London has been called to a paper or prospectus of a "Society for the Encouragement of Literature and Science," whereof W. Serjeant-Rodway is stated to be "Secretary-in-Chief," and wherein the names Lewis Biden, A. Ware, and Joseph Blackburn Leslie each appear followed by the letters F.L.S., which letters are those appointed to indicate "Fellow of the Linnean Society"—a chartered society. Its attention has also been called to another paper apparently put forth by a "Conchological Society of London," wherein the name W. Serjeant-Rodway appears as "Secretary and Founder," with the addition of the letters F.L.S. (Lond.).

As no one of these four gentlemen is a Fellow of the Linnean Society, the Council of the same Society has requested me to make the fact known, and I shall therefore be much obliged if you will be so kind as to give insertion to this letter in NATURE.

Linnean Society, Burlington House, ST. GEORGE MIVART
Piccadilly, W., November 27 Zool. Sec. Lin. Soc.

Does Sargassum Vegetate in the Open Sea?

If the correspondent in NATURE, vol. xxi. p. 80, under the above title, would again refer to my communication in vol. xx. p. 578, which I much regret he finds so unsatisfactory, he will

see that the several statements and quotations it contains are exclusively based upon "personal" observations made by myself and by the naturalists on board the *Challenger* during our cruise in the North Atlantic in the year 1873. In replying to his inquiries in vol. xx. p. 552, I was only anxious to supply him with what I considered to be the latest and the most reliable information available on the subject, and which hardly deserves to be qualified as "old reports" and as "a mixture of the prevalent opinion since Columbus and observed facts."

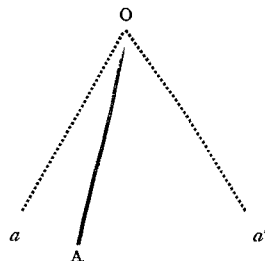
The term *Sargasso Sea* has been extended by geographers, and not incorrectly so, to all oceanic areas where large aggregations of sea-weed are met with, and it does not necessarily imply the presence of Sargassum, i.e., *Sargassum bacciferum* in these regions, since the original Spanish word *Sargazo* (in Portuguese *Sargazo*) simply means "sea-weed." I am, therefore, not surprised that the correspondent should not have found any gulf-weed while crossing the Pacific Sargasso Sea.

Nor can the obscurity in which so many details connected with the gulf-weed are still involved be fairly ascribed to want of observation on the part of the few naturalists who have had the opportunity of studying this interesting alga *in situ*, that is to say, in the middle of the North Atlantic, but rather to the great difficulty, if not impossibility, of ascertaining the life-history of a specimen accidentally found floating on the surface of the ocean. For this reason I fear that some time may elapse before the numerous questions put by the correspondent in vol. xxi. pp. 80-81 can be satisfactorily answered. A botanist stationed for several seasons at Bermudas, or on one of the Bahama Islands, might probably succeed in throwing some light upon the successive stages in the growth and decay of *Sargassum bacciferum*.

J. J. WILD

The Paces of the Horse

I VENTURE to offer the following illustration of the effect produced on the eye by a horse galloping.



I take a pencil, O A, and oscillate it rapidly between the positions O a and O a'. The impression produced on my eye is an indistinct fan-shaped figure, a O a', bounded by two rather distinct images of the pencil in its extreme positions O a and O a'. The indistinctness of the fan-shaped figure is caused by the rapid change of position of the pencil, which is reduced to a minimum at O a and O a', where the pencil swings up to, and returns back from, its extreme positions, passing over the same ground twice in successive instants of time, and thus seeming to pause in the immediate neighbourhood of those positions. An artist representing this effect would draw the indistinct fan-shaped figure; and the two rather distinct images of the pencil at O a and O a'.

The relative motion of the legs of a horse galloping may be looked upon as that of rapidly oscillating pendulums with this very important addition; that besides their pendulum-like oscillations they go through rapid internal changes of form, owing to the bending, or doubling up, of the legs at the knees, hocks, and fetlocks, at every stride. The rapidity of these internal changes is reduced to a minimum when the leg is in its extreme outstretched position. Again, it is in this same position that the rapidity of change of position owing to the pendulum-like oscillation is also at a minimum. The two minimums are, as it were, coincident, and, as a consequence, every leg as it reaches its outstretched position, seems for an instant to pause, leaving a rather distinct impression on the eye. The other legs on successively reaching their respective outstretched positions produce corresponding impressions on the eye. It is a fact that the legs do not reach these positions simultaneously; they reach them successively, but the image produced by one leg in its outstretched position has not time to be obliterated before the images of the other legs are produced in their corresponding outstretched positions. Therefore they appear to us to be all simultaneously in those out-

stretched positions, and it is thus that the artist should represent them. It is his duty to represent things as they *appear*, rather than as they actually *are*, at a given instant of time.

The fan-shaped form noticed in the case of the oscillating pencil becomes exceedingly indistinct, if it does not disappear altogether, in the case of the galloping horse's legs. This is owing to the rapid internal changes of form of the legs.

Your correspondent, Sir W. G. Simpson, Bart., states in his excellent letter produced in *NATURE*, vol. xxi. p. 55, that a galloping horse might be represented with all its legs gathered under it. I venture to disagree with him for this reason: the two "minimums" to which I have referred in a former part of this letter are *not* coincident when the legs are in their extreme position gathered under the body, and therefore no such distinct image of them in that position is produced. The "minimums" are only coincident in the other extreme, viz., the outstretched position.

The artistic representation of a horse's paces other than galloping, as also that of other objects in motion, can be determined by similar reasoning. V. B. BARRINGTON-KENNETT
15, Hyde Park Gardens, W., November 26

Force and Momentum

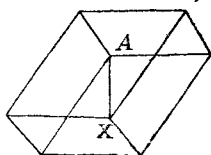
It is commonly said that change of momentum is evidence that force has acted or is acting on the mass, and that the rate at which the momentum is changing is the measure of the force. Thus, in his lecture on "Force," Prof. Tait says: "Force is the rate of change of momentum" (*NATURE*, vol. xiv. p. 462). This is not true if the mass be variable. Suppose a sphere of ice moving with constant velocity in a straight line through hot space. The mass, and therefore the momentum, is changing at every instant by the evaporation of the ice. The evaporation being supposed uniform over the entire surface, any force impressed on the sphere by the mutual repulsion between it and a particle of vapour thrown off at a point, p , is balanced by an equal force at the other end of the diameter through p . Hence, the resultant of all these forces is nothing. Here, then, we have change of momentum of the sphere, although no force acts on it. In like manner the change of momentum of a rocket and of a locomotive engine is partly due to change of mass. Does it not hence appear that change of *velocity* is the proper evidence of the action of force? When a variable mass, m , is in motion, the proper measure of the force acting on m at any given instant in any given direction is—not the rate of change of momentum, but—the product of the value of the mass at that instant, and the value of the rate of change of the velocity at that instant and in that direction, *i.e.*, the measure of the force is not $\frac{d(mv)}{dt}$, but $m \frac{dv}{dt}$.

E. G.

[There is no such thing in nature as a "variable mass"; and our correspondent's difficulty arises from his omitting to take account of the momentum of each of the parts (however small) into which a mass may be divided. In most good works on dynamics he will find the motion of a rocket, or of a descending rain-drop (which gathers mass as it falls), accurately treated on the assumption that the momentum produced per unit of time is the measure of the force acting.—ED.]

Change in Apparent Position of Geometrical Figures

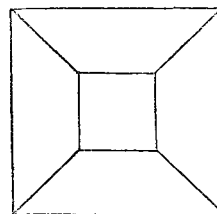
THE perplexing illusion to which Mr. Bellamy refers (*NATURE*, vol. xx. p. 362) has long been known, and various explanations have been given of it by physicists. Sir Chas. Wheatstone, in 1838, showed clearly that it is a mental operation, while combating the idea of Prof. Necker, of Geneva, who attri-



buted the alteration of appearance in geometrical figures, not to a mental operation, but to an involuntary change in the adjustment of the eye for obtaining distinct vision. Necker's experiment is substantially the same as that described by Mr. Bellamy. The solid angles at A and X being alternately looked at, sometimes one and sometimes the other appears the nearer, the entire figure at the same time changing in unison; and as Wheatstone points

out, "the change of figure frequently occurs while the eye continues to look at the same angle."

In the following experiment it is seen more clearly still that the operation is a mental one, because there is neither movement of recti, oblique, nor ciliary muscles. Two concentric squares have their corners joined by straight lines. The lesser square



will appear in a plane anterior, or posterior to the larger, according as we regard the figure as the representation of a truncated pyramid, or as the representation of a room with its sides all sloping away to the distant square wall. Here no eye muscles are concerned; the image on the retina remains unaltered, and the only operation is a mental one, a turning to the results of past experience. WM. ACKROYD

Mutual Attraction of Spectral Lines

I DO not know that it has been remarked that a line in the diffraction-spectrum (whether bright or dark) must be shifted from its normal position in case another line falls very near it. Neighbouring lines must be attracted if both are bright or both dark, and repelled if one is bright and the other dark. The reason is that the lines are only maxima or minima of light, and the differential coefficient of the sum does not vanish at the same points as the differential coefficients of the separate terms. The shifting will be the greatest in the case of a faint line near a very intense one. I have succeeded in this way in shifting the positions of lines by measurable amounts ($1'$ to $2''$).

Baltimore, Md., November 14

C. S. PEIRCE

EXPLORATION OF TIMOR

IT will be perhaps of some interest to the readers of *NATURE* to hear that Mr. Riedel, the Dutch resident on Timor (Timor Kupang), who formerly lived on Celebes, and collected a great deal on this island for European museums, and who is known by his various writings on different scientific questions concerning the East, has just returned from a twenty-five days' journey through Central Timor from $123^{\circ} 30'$ — 125° E.L., as he wrote to me in a letter dated October 6. No European has made such a journey through Timor before, and it has been very troublesome. But the country is, Mr. Riedel remarks, a splendid one, and very suitable for coffee and cinchona cultivation. The traveller did not see any Negritos, who, according to the assertion of M. Hamy, live in the interior of Timor, nor did he hear anything of a Casuary which was reported from there recently. Mr. Riedel collected many geographical notes, and sketched a map of the parts which he visited. A small collection of plants was forwarded to me by Mr. Riedel, and I have sent them to Kew, as Prof. Oliver formerly had the kindness to determine several botanical collections of Mr. Riedel's from Celebes. A. B. MEYER

Dresden, November 29,
Royal Zool. Museum

LAND SHELLS OF THE AUSTRAL ISLANDS

THE small island of *Rurutu* (Oheatora of Capt. Cook) is about 320 miles south-south-west of Tahiti; it is eight miles in length, and has an elevation of 1,500 feet, over 100 feet consisting of old coral reefs which have been upheaved to that altitude. Mr. Charles de Gage, a resident and experienced naturalist, has collected a number of land-shells, which have been studied